OBITUARY NOTICES OF FELLOWS DECEASED.

Charles Robert Darwin was the fifth child and second son of Robert Waring Darwin and Susannah Wedgwood, and was born on the 12th February, 1809, at Shrewsbury, where his father was a physician in large practice.

Mrs. Robert Darwin died when her son Charles was only eight years old, and he hardly remembered her. A daughter of the famous Josiah Wedgwood, who created a new branch of the potter's art, and established the great works of Etruria, could hardly fail to transmit important mental and moral qualities to her children; and there is a solitary record of her direct influence in the story told by a schoolfellow, who remembers Charles Darwin "bringing a flower to school, and saying that his mother had taught him how, by looking at the inside of the blossom, the name of the plant could be discovered." (I, p. 28.*)

The theory that men of genius derive their qualities from their mothers, however, can hardly derive support from Charles Darwin's case, in the face of the patent influence of his paternal forefathers. Dr. Darwin, indeed, though a man of marked individuality of character, a quick and acute observer, with much practical sagacity, is said not to have had a scientific mind. But when his son adds that his father "formed a theory for almost everything that occurred" (I, p. 20), he indicates a highly probable source for that inability to refrain from forming an hypothesis on every subject which he confesses to be one of the leading characteristics of his own mind, some pages further on (I, p. 103). Dr. R. W. Darwin, again, was the third son of Erasmus Darwin, also a physician of great repute, who shared the intimacy of Watt and Priestley, and was widely known as the author of 'Zoonomia,' and other voluminous poetical and prose works which had a great vogue in the latter half of the eighteenth century. The celebrity which they enjoyed was in part due to the attractive style (at least according to the taste of that day) in which the author's extensive, though not very profound, acquaintance with natural phenomena was set forth; but in a still greater degree, probably, to the boldness of the speculative views, always ingenious and sometimes fantastic, in which he indulged. The conception of evolution set afoot by De Maillet and others, in the early part of the century, not only found a vigorous champion in

* The references throughout this notice are to the 'Life and Letters,' unless the contrary is expressly stated.

Erasmus Darwin ; but he propounded an hypothesis as to the manner in which the species of animals and plants have acquired their characters, which is identical in principle with that subsequently rendered famous by Lamarck.

That Charles Darwin's chief intellectual inheritance came to him from the paternal side then, is hardly doubtful. But there is nothing to show that he was, to any sensible extent, directly influenced by his grandfather's biological work. He tells us that a perusal of the 'Zoonomia' in early life produced no effect upon him, although he greatly admired it—and that on reading it again, ten or fifteen years afterwards, he was much disappointed, "the proportion of speculation being so large to the facts given." But with his usual anxious candour he adds, "Nevertheless, it is probable that the hearing, rather early in life, such views maintained and praised, may have favoured my upholding them, in a different form, in my 'Origin of Species.'" (I, p. 38.) Erasmus Darwin was in fact an anticipator of Lamarck, and not of Charles Darwin ; there is no trace in his works of the conceptions by the addition of which his grandson metamorphosed the theory of evolution as applied to living things and gave it a new foundation.

Charles Darwin's childhood and youth afforded no intimation that he would be, or do, anything out of the common run. In fact, the prognostications of the educational authorities into whose hands he first fell, were most distinctly unfavourable ; and they counted the only boy of original genius who is known to have come under their hands as no better than a dunce. The history of the educational experiments to which Darwin was subjected is curious, and not without a moral for the present generation. There were four of them, and three were failures. Yet it cannot be said that the materials on which the pedagogic powers operated were other than good. In his boyhood, Darwin was strong, well-grown, and active, taking the keen delight in field sports and in every description of hard physical exercise which is natural to an English country-bred lad ; and, in respect of things of the mind, he was neither apathetic, nor idle, nor one-sided. The 'Autobiography' tells us that he "had much zeal for whatever interested" him, and he was interested in many and very diverse topics. He could work hard, and liked a complex subject better than an easy one. The "clear geometrical proofs" of Euclid delighted him. His interest in practical chemistry, carried out in an extemporised laboratory, in which he was permitted to assist by his elder brother, kept him late at work, and earned him the nickname of "gas" among his schoolfellows. And there could have been no insensibility to literature in one who, as a boy, could sit for hours reading Shakespeare, Milton, Scott, and Byron ; who greatly admired some of the Odes of Horace ; and who, in later years, on board the

"Beagle," when only one book could be carried on an expedition, chose a volume of Milton for his companion.

Industry, intellectual interests, the capacity for taking pleasure in deductive reasoning, in observation, in experiment, no less than in the highest works of imagination: where these qualities are present any rational system of education should surely be able to make something of them. Unfortunately for Darwin, the Shrewsbury Grammar School, though good of its kind, was an institution of a type universally prevalent in this country half a century ago, and by no means extinct at the present day. The education given was "strictly classical," "especial attention" being "paid to verse-making," while all other subjects, except a little ancient geography and history, were ignored. Whether, as in some famous English schools at that date and much later, elementary arithmetic was also left out of sight does not appear; but the instruction in Euclid which gave Charles Darwin so much satisfaction was certainly supplied by a private tutor. That a boy, even in his leisure hours, should permit himself to be interested in any but book-learning seems to have been regarded as little better than an outrage by the head master, who thought it his duty to administer a public rebuke to young Darwin for wasting his time on such a contemptible subject as chemistry. English composition and literature, modern languages, modern history, modern geography, appear to have been considered to be as despicable as chemistry.

For seven long years, Darwin got through his appointed tasks; construed without cribs, learned by rote whatever was demanded, and concocted his verses in approved schoolboy fashion. And the result, as it appeared to his mature judgment, was simply negative. "The school as a means of education to me was simply a blank." (I, p. 32.) On the other hand, the extraneous chemical exercises, which the head master treated so contumeliously, are gratefully spoken of as the "best part" of his education while at school. Such is the judgment of the scholar on the school; as might be expected, it has its counterpart in the judgment of the school on the scholar. The collective intelligence of the staff of Shrewsbury School could find nothing but dull mediocrity in Charles Darwin. The mind that found satisfaction in knowledge, but very little in mere learning, that could appreciate literature, but had no particular aptitude for grammatical exercises, appeared to the "strictly classical" pedagogue to be no mind at all. As a matter of fact, Darwin's school education left him ignorant of almost all the things which it would have been well for him to know, and untrained in all the things it would have been useful for him to be able to do, in after life. Drawing, practice in English composition, and instruction in the elements of the physical sciences, would not only have been infinitely valuable to him in reference to his future career, but would have furnished

the discipline suited to his faculties, whatever that career might be. And a knowledge of French and German, especially the latter, would have removed from his path obstacles which he never fully overcame.

Thus, starved and stunted on the intellectual side, it is not surprising that Charles Darwin's energies were directed towards athletic amusements and sport, to such an extent, that even his kind and sagacious father could be exasperated into telling him that "he cared for nothing but shooting, dogs, and rat-catching." (I, p. 32.) It would be unfair to expect even the wisest of fathers to have foreseen that the shooting and the rat-catching, as training in the ways of quick observation and in physical endurance, would prove more valuable than the construing and verse-making to his son, whose attempt, at a later period of his life, to persuade himself "that shooting was almost an intellectual employment: it required so much skill to judge where to find most game, and to hunt the dogs well" (I, p. 43), was by no means so sophistical as he seems to have been ready to admit.

In 1825, Dr. Darwin came to the very just conclusion that his son Charles would do no good by remaining at Shrewsbury School, and sent him to join his elder brother Erasmus, who was studying medicine at Edinburgh, with the intention that the younger son should also become a medical practitioner. Both sons, however, were well aware that their inheritance would relieve them from the urgency of the struggle for existence which most professional men have to face, and they seem to have allowed their tastes, rather than the medical curriculum, to have guided their studies. Erasmus Darwin was debarred by constant ill-health from seeking the public distinction which his high intelligence and extensive knowledge would, under ordinary circumstances, have insured. He took no great interest in biological subjects, but his companionship must have had its influence on his brother. Still more was exerted by friends like Coldstream and Grant, both subsequently well-known zoologists (and the latter an enthusiastic Lamarckian), by whom Darwin was induced to interest himself in marine zoology. A notice of the ciliated germs of *Flustra*, communicated to the Plinian Society in 1826, was the first fruits o' Darwin's half century of scientic work. Occasional attendance at the Wernerian Society brought him into relation with that excellent ornithologist the elder Macgillivray, and enabled him to see and hear Audubon. Moreover, he got lessons in bird-stuffing from a negro, who had accompanied the eccentric traveller Waterton in his wanderings, before settling in Edinburgh.

No doubt Darwin picked up a great deal of valuable knowledge during his two years' residence in Scotland; but it is equally clear that next to none of it came through the regular channels of academic education. Indeed, the influence of the Edinburgh professoriate

appears to have been mainly negative, and in some cases deterrent; creating in his mind, not only a very low estimate of the value of lectures, but an antipathy to the subjects which had been the occasion of the boredom inflicted upon him by their instrumentality. With the exception of Hope, the Professor of Chemistry, Darwin found them all "intolerably dull." Forty years afterwards he writes of the lectures of the Professor of Materia Medica that they were "fearful to remember." The Professor of Anatomy made his lectures "as dull as he was himself," and he must have been very dull to have wrung from his victim the sharpest personal remark recorded as his. But the climax seems to have been attained by the Professor of Geology and Zoology, whose prælections were so "incredibly dull" that they produced in their hearer the somewhat rash determination never "to read a book on geology or in any way to study the science" so long as he lived. (I, p. 41.)

There is much reason to believe that the lectures in question were eminently qualified to produce the impression which they made; and there can be little doubt, that Darwin's conclusion that his time was better employed in reading than in listening to such lectures was a sound one. But it was particularly unfortunate that the personal and professorial dulness of the Professor of Anatomy, combined with Darwin's sensitiveness to the disagreeable concomitants of anatomical work, drove him away from the dissecting room. In after life, he justly recognised that this was an "irremediable evil" in reference to the pursuits he eventually adopted; indeed, it is marvellous that he succeeded in making up for his lack of anatomical discipline, so far as his work on the Cirripedes shows he did. And the neglect of anatomy had the further unfortunate result that it excluded him from the best opportunity of bringing himself into direct contact with the facts of nature which the University had to offer. In those days, almost the only practical scientific work accessible to students was anatomical, and the only laboratory at their disposal the dissecting room.

We may now console ourselves with the reflection that the partial evil was the general good. Darwin had already shown an aptitude for practical medicine (I, p. 37); and his subsequent career proved that he had the making of an excellent anatomist. Thus, though his horror of operations would probably have shut him off from surgery, there was nothing to prevent him (any more than the same peculiarity prevented his father) from passing successfully through the medical curriculum and becoming, like his father and grandfather, a successful physician, in which case 'The Origin of Species' would not have been written. Darwin has jestingly alluded to the fact that the shape of his nose (to which Captain Fitzroy objected), nearly prevented his embarkation in the "Beagle"; it may be that the sensitiveness of that organ secured him for science.

At the end of two years' residence in Edinburgh, it hardly needed Dr. Darwin's sagacity to conclude that a young man, who found nothing but dulness in professorial lucubrations, could not bring himself to endure a dissecting room, fled from operations, and did not need a profession as a means of livelihood, was hardly likely to distinguish himself as a student of medicine. He therefore made a new suggestion, proposing that his son should enter an English University and qualify for the ministry of the Church. Charles Darwin found the proposal agreeable, none the less, probably, that a good deal of natural history and a little shooting were by no means held, at that time, to be incompatible with the conscientious performance of the duties of a country clergyman. But it is characteristic of the man, that he asked time for consideration, in order that he might satisfy himself that he could sign the Thirty-nine Articles with a clear conscience. However, the study of "Pearson on the Creeds" and a few other books of divinity soon assured him that his religious opinions left nothing to be desired on the score of orthodoxy, and he acceded to his father's proposition.

The English University selected was Cambridge; but an unexpected obstacle arose from the fact that, within the two years which had elapsed since the young man who had enjoyed seven years of the benefit of a strictly classical education had left school, he had forgotten almost everything he had learned there, "even to some few of the Greek letters." (I, p. 46.) Three months with a tutor, however, brought him back to the point of translating Homer and the Greek Testament "with moderate facility," and Charles Darwin commenced the third educational experiment of which he was the subject, and was entered on the books of Christ's College in October 1827. So far as the direct results of the academic training thus received are concerned, the English University was not more successful than the Scottish. "During the three years which I spent at Cambridge my time was wasted, as far as the academical studies were concerned, as completely as at Edinburgh and as at school." (I, p. 46.) And yet, as before, there is ample evidence that this negative result cannot be put down to any native defect on the part of the scholar. Idle and dull young men, or even young men who being neither idle nor dull, are incapable of caring for anything but some hobby, do not devote themselves to the thorough study of Paley's 'Moral Philosophy,' and 'Evidences of Christianity'; nor are their reminiscences of this particular portion of their studies expressed in terms such as the following: "The logic of this book [the 'Evidences'] and, as I may add, of his 'Natural Theology' gave me as much delight as did Euclid." (I, p. 47.)

The collector's instinct, strong in Darwin from his childhood, as is usually the case in great naturalists, turned itself in the direction of Insects during his residence at Cambridge. In childhood, it had been

damped by the moral scruples of a sister, as to the propriety of catching and killing insects for the mere sake of possessing them, but now it broke out afresh, and Darwin became an enthusiastic beetle collector. Oddly enough he took no scientific interest in beetles, not even troubling himself to make out their names; his delight lay in the capture of a species which turned out to be rare or new, and still more in finding his name, as captor, recorded in print. Evidently, this beetle-hunting hobby had little to do with science, but was mainly a new phase of the old and undiminished love of sport. In the intervals of beetle-catching, when shooting and hunting were not to be had, riding across country answered the purpose. These tastes naturally threw the young undergraduate among a set of men who prefered hard riding to hard reading, and wasted the midnight oil upon other pursuits than that of academic distinction. A superficial observer might have had some grounds to fear that Dr. Darwin's wrathful prognosis might yet be verified. But if the eminently social tendencies of a vigorous and genial nature sought an outlet among a set of jovial sporting friends, there were other and no less strong proclivities which brought him into relation with associates of a very different stamp.

Though almost without ear and with a very defective memory for music, Darwin was so strongly and pleasurably affected by it that he became a member of a musical society; and an equal lack of natural capacity for drawing did not prevent him from studying good works of art with much care.

An acquaintance with even the rudiments of physical science was no part of the requirements for the ordinary Cambridge degree. But there were professors both of Geology and of Botany whose lectures were accessible to those who chose to attend them. The occupants of these chairs, in Darwin's time, were eminent men and also admirable lecturers in their widely different styles. The horror of geological lectures which Darwin had acquired at Edinburgh, unfortunately prevented him from going within reach of the fervid eloquence of Sedgwick; but he attended the botanical course, and though he paid no serious attention to the subject, he took great delight in the country excursions, which Henslow so well knew how to make both pleasant and instructive. The Botanical Professor was, in fact, a man of rare character and singularly extensive acquirements in all branches of natural history. It was his greatest pleasure to place his stores of knowledge at the disposal of the young men who gathered about him, and who found in him, not merely an encyclopedic teacher but a wise counseller, and, in case of worthiness, a warm friend. Darwin's acquaintance with him soon ripened into a friendship which was terminated only by Henslow's death in 1861, when his quondam pupil gave touching expression to his sense of what he

owed to one whom he calls (in one of his letters) his "dear old master in Natural History." (II, p. 217.) It was by Henslow's advice that Darwin was led to break the vow he had registered against making an acquaintance with geology; and it was through Henslow's good offices with Sedgwick that he obtained the opportunity of accompanying the Geological Professor on one of his excursions in Wales. He then received a certain amount of practical instruction in Geology, the value of which he subsequently warmly acknowledged. (I, p. 237.) In another direction, Henslow did him an immense, though not altogether intentional service, by recommending him to buy and study the recently published first volume of Lyell's 'Principles.' As an orthodox geologist of the then dominant catastrophic school, Henslow accompanied his recommendation with the admonition on no account to adopt Lyell's general views. But the warning fell on deaf ears, and it is hardly too much to say that Darwin's greatest work is the outcome of the unflinching application to Biology of the leading idea and the method applied in the 'Principles' to Geology.* Finally, it was through Henslow, and at his suggestion, that Darwin was offered the appointment to the "Beagle" as naturalist.

During the latter part of Darwin's residence at Cambridge the prospect of entering the Church, though the plan was never formally renounced, seems to have grown very shadowy. Humboldt's 'Personal Narrative,' and Herschel's 'Introduction to the Study of Natural Philosophy,' fell in his way and revealed to him his real vocation. The impression made by the former work was very strong. "My whole course of life," says Darwin in sending a message to Humboldt, "is due to having read and re-read, as a youth, his personal narrative." (I, p. 336.) The description of Teneriffe inspired Darwin with such a strong desire to visit the island, that he took some steps towards going there—inquiring about ships, and so on.

But, while this project was fermenting, Henslow, who had been asked to recommend a naturalist for Captain Fitzroy's projected expedition, at once thought of his pupil. In his letter of the 24th August, 1831, he says: "I have stated that I consider you to be the best qualified person I know of who is likely to undertake such a situation. I state this—not on the supposition of your being a *finished* naturalist, but as amply qualified for collecting, observing, and noting anything worthy to be noted in Natural History . . . . The voyage is to

* "After my return to England it appeared to me that by following the example of Lyell in Geology, and by collecting all facts which bore in any way on the variation of animals and plants under domestication and nature, some light might perhaps be thrown on the whole subject [of the origin of species]." (I, p. 83.) See also the dedication of the second edition of the 'Journal of a Naturalist.'

last two years, and if you take plenty of books with you, anything you please may be done." (I, p. 193.) The state of the case could not have been better put. Assuredly the young naturalist's theoretical and practical scientific training had gone no further than might suffice for the outfit of an intelligent collector and notetaker. He was fully conscious of the fact, and his ambition hardly rose above the hope that he should bring back materials for the scientific "lions" at home of sufficient excellence to prevent them from turning and rending him. (I, p. 248.)

But a fourth educational experiment was to be tried. This time Nature took him in hand herself and showed him the way by which, to borrow Henslow's prophetic phrase, "anything he pleased might be done."

The conditions of life presented by a ship-of-war of only 242 tons burthen, would not, *primâ facie*, appear to be so favourable to intellectual development as those offered by the cloistered retirement of Christ's College. Darwin had not even a cabin to himself; while, in addition to the hindrances and interruptions incidental to sea-life, which can be appreciated only by those who have had experience of them, sea-sickness came on whenever the little ship was "lively"; and, considering the circumstances of the cruise, that must have been her normal state. Nevertheless, Darwin found on board the "Beagle" that which neither the pedagogues of Shrewsbury, nor the professoriate of Edinburgh, nor the tutors of Cambridge had managed to give him. "I have always felt that I owe to the voyage the first real training or education of my mind (I, p. 61);" and in a letter, written as he was leaving England, he calls the voyage on which he was starting, with just insight, his "second life." (I, p. 214.) Happily for Darwin's education, the school-time of the "Beagle" lasted five years instead of two; and the countries which the ship visited were singularly well fitted to provide him with object-lessons on the nature of things of the greatest value.

While at sea, he diligently collected, studied, and made copious notes upon the surface Fauna. But with no previous training in dissection, hardly any power of drawing, and next to no knowledge of comparative anatomy, his occupation with work of this kind—notwithstanding all his zeal and industry—resulted, for the most part, in a vast accumulation of useless manuscript. Some acquaintance with the marine *Crustacea*, observations on *Planariæ* and on the ubiquitous *Sagitta*, seem to have been the chief results of a great amount of labour in this direction.

It was otherwise with the terrestrial phenomena which came under the voyager's notice: and Geology very soon took her revenge for the scorn which the much-bored Edinburgh student had poured upon her. Three weeks after leaving England the ship touched land for the

first time at St. Jago, in the Cape de Verd Islands, and Darwin found his attention vividly engaged by the volcanic phenomena and the signs of upheaval which the island presented. His geological studies had already indicated the direction in which a great deal might be done, beyond collecting; and it was while sitting beneath a low lava cliff on the shore of this island, that a sense of his real capability first dawned upon Darwin, and prompted the ambition to write a book on the geology of the various countries visited. (I, p. 66.) Even at this early date, Darwin must have thought much on geological topics, for he was already convinced of the superiority of Lyell's views to those entertained by the catastrophists*; and his subsequent study of the tertiary deposits and of the terraced gravel beds of South America was eminently fitted to strengthen that conviction. The letters from South America contain little reference to any scientific topic except geology; and even the theory of the formation of coral reefs was prompted by the evidence of extensive and gradual changes of level afforded by the geology of South America; "No other work of mine," he says, "was begun in so deductive a spirit as this; for the whole theory was thought out on the West Coast of South America, before I had seen a true coral reef. I had, therefore, only to verify and extend my views by a careful examination of living reefs." (I, p. 70.) In 1835, when starting from Lima for the Galapagos, he recommends his friend, W. D. Fox, to take up geology:—"there is so much larger a field for thought than in the other branches of Natural History. I am become a zealous disciple of Mr. Lyell's views, as made known in his admirable book. Geologising in South America, I am tempted to carry parts to a greater extent even than he does. Geology is a capital science to begin with, as it requires nothing but a little reading, thinking, and hammering." (I, p. 263.) The truth of the last statement, when it was written, is a curious mark of the subsequent progress of geology. Even so late as 1836, Darwin speaks of being "much more inclined for geology than the other branches of Natural History." (I, p. 275.)

At the end of the letter to Mr. Fox, however, a little doubt is expressed whether zoological studies might not, after all, have been more profitable; and an interesting passage in the Autobiography enables us to understand the origin of this hesitation.

"During the voyage of the 'Beagle' I had been deeply impressed

* "I had brought with me the first volume of Lyell's 'Principles of Geology,' which I studied attentively; and the book was of the highest service to me in many ways. The very first place which I examined, namely, St. Jago in the Cape de Verd Islands, showed me clearly the wonderful superiority of Lyell's manner of treating Geology, compared with that of any other author whose works I had with me or ever afterwards read." (I, p. 62.)

by discovering in the Pampean formation great fossil animals covered with armour like that on the existing armadillos; secondly, by the manner in which closely-allied animals replace one another in proceeding southwards over the continent; and, thirdly, by the South American character of most of the productions of the Galapagos Archipelago, and, more especially, by the manner in which they differ slightly on each island of the group: some of the islands appearing to be very ancient in a geological sense.

"It was evident that such facts as these, as well as many others, could only be explained on the supposition that species gradually become modified; and the subject haunted me. But it was equally evident that neither the action of the surrounding conditions, nor the will of the organisms (especially in the case of plants) could account for the innumerable cases in which organisms of every kind are beautifully adapted to their habits of life; for instance, a woodpecker or a tree-frog to climb trees, or a seed for dispersal by hooks or plumes. I had always been much struck by such adaptations, and until these could be explained it seemed to me almost useless to endeavour to prove by indirect evidence that species have been modified." (I, p. 82.)

The facts to which reference is here made were, without doubt, eminently fitted to attract the attention of a philosophical thinker; but until the relations of the existing with the extinct species and of the species of the different geographical areas with one another were determined with some exactness, they afforded but an unsafe foundation for speculation. It was not possible that this determination should have been effected before the return of the "Beagle" to England; and thus the date which Darwin (writing in 1837) assigns to the dawn of the new light which was rising in his mind becomes intelligible.*

"In July opened first note-book on Transmutation of Species. Had been greatly struck from about the month of previous March on character of South American fossils and species on Galapagos Archipelago. These facts (especially latter) origin of all my views." (I, p. 276.)

* I am indebted to Mr. F. Darwin for the knowledge of a letter addressed by his father to Dr. Otto Zacharias in 1877, which contains the following paragraph, confirmatory of the view expressed above: "When I was on board the 'Beagle,' I believed in the permanence of species, but, as far as I can remember, vague doubts occasionally flitted across my mind. On my return home in the autumn of 1836 I immediately began to prepare my journal for publication, and then saw how many facts indicated the common descent of species, so that in July, 1837, I opened a note-book to record any facts which might bear on the question. But I did not become convinced that species were mutable until I think two or three years had elapsed."

From March, 1837, then, Darwin, not without many misgivings and fluctuations of opinion, inclined towards transmutation as a provisional hypothesis. Three months afterwards he is hard at work collecting facts for the purpose of testing the hypothesis; and an almost apologetic passage in a letter to Lyell shows that, already, the attractions of biology are beginning to predominate over those of geology.

"I have lately been sadly tempted to be idle*—that is, as far as pure Geology is concerned—by the delightful number of new views which have been coming in thickly and steadily—on the classification and affinities and instincts of animals—bearing on the question of species. Note-book after note-book has been filled with facts which begin to group themselves *clearly* under sub-laws." (I, p. 298.)

The problem which was to be Darwin's chief subject of occupation for the rest of his life thus presented itself, at first, mainly under its distributional aspect. Why do species present certain relations in space and in time? Why are the animals and plants of the Galapagos Archipelago so like those of South America and yet different from them? Why are those of the several islets more or less different from one another? Why are the animals of the latest geological epoch in South America similar in *facies* to those which exist in the same region at the present day, and yet specifically or generically different?

The reply to these questions, which was almost universally received fifty years ago, was that animals and plants were created such as they are; and that their present distribution, at any rate so far as terrestrial organisms are concerned, has been effected by the migration of their ancestors from the region in which the ark stranded after the subsidence of the deluge. It is true that the geologists had drawn attention to a good many tolerably serious difficulties in the way of the diluvial part of this hypothesis, no less than to the supposition that the work of creation had occupied only a brief space of time. But even those, such as Lyell, who most strenuously argued in favour of the sufficiency of natural causes for the production of the phenomena of the inorganic world, held stoutly by the hypothesis of creation in the case of those of the world of life.

For persons who were unable to feel satisfied with the fashionable doctrine, there remained only two alternatives—the hypothesis of spontaneous generation, and that of descent with modification. The former was simply the creative hypothesis with the creator left out; the latter had already been propounded by De Maillet and Erasmus Darwin, among others, and, later, systematically expounded by

* Darwin generally uses the word "idle" in a peculiar sense. He means by it working hard at something he likes when he ought to be occupied with a less attractive subject. Though it sounds paradoxical, there is a good deal to be said in favour of this view of pleasant work.

Lamarck. But in the eyes of the naturalist of the "Beagle" (and, probably, in those of most sober thinkers), the advocates of transmutation had done the doctrine they expounded more harm than good.

Darwin's opinion of the scientific value of the 'Zoonomia' has already been mentioned. His verdict on Lamarck is given in the following passage of a letter to Lyell (March, 1863):—

"Lastly, you refer repeatedly to my view as a modification of Lamarck's doctrine of development and progression. If this is your deliberate opinion there is nothing to be said, but it does not seem so to me. Plato, Buffon, my grandfather, before Lamarck and others, propounded the *obvious* view that if species were not created separately they must have descended from other species, and I can see nothing else in common between the 'Origin' and Lamarck. I believe this way of putting the case is very injurious to its acceptance, as it implies necessary progression, and closely connects Wallace's and my views with what I consider, after two deliberate readings, as a wretched book, and one from which (I well remember to my surprise) I gained nothing."

"But," adds Darwin with a little touch of banter, "I know you rank it higher, which is curious, as it did not in the least shake your belief." (III, p. 14; see also p. 16, "to me it was an absolutely useless book.")

Unable to find any satisfactory theory of the process of descent with modification in the works of his predecessors, Darwin proceeded to lay the foundations of his own views independently; and he naturally turned, in the first place, to the only certainly known examples of descent with modification, namely, those which are presented by domestic animals and cultivated plants. He devoted himself to the study of these cases with a thoroughness to which none of his predecessors even remotely approximated; and he very soon had his reward in the discovery "that selection was the keystone of man's success in making useful races of animals and plants." (I, p. 83.)

This was the first step in Darwin's progress, though its immediate result was to bring him face to face with a great difficulty. "But how selection could be applied to organisms living in a state of nature remained for some time a mystery to me." (I, p. 83.)

The key to this mystery was furnished by the accidental perusal of the famous essay of Malthus 'On Population' in the autumn of 1838. The necessary result of unrestricted multiplication is competition for the means of existence. The success of one competitor involves the failure of the rest, that is, their extinction; and this "selection" is dependent on the better adaptation of the successful competitor to the conditions of the competition. Variation occurs under natural, no less than under artificial, conditions. Unrestricted multiplication

implies the competition of varieties and the selection of those which are relatively best adapted to the conditions.

Neither Erasmus Darwin, nor Lamarck, had any inkling of the possibility of this process of "natural selection"; and though it had been foreshadowed by Wells in 1813, and more fully stated by Matthew in 1831, the speculations of the latter writer remained unknown to naturalists until after the publication of the 'Origin of Species.'

Darwin found in the doctrine of the selection of favourable variations by natural causes, which thus presented itself to his mind, not merely a probable theory of the origin of the diverse species of living forms, but that explanation of the phenomena of adaptation, which previous speculations had utterly failed to give. The process of natural selection is, in fact, dependent on adaptation—it is all one, whether one says that the competitor which survives is the "fittest" or the "best adapted." And it was a perfectly fair deduction that even the most complicated adaptations might result from the summation of a long series of simple favourable variations.

Darwin notes as a serious defect in the first sketch of his theory that he had omitted to consider one very important problem, the solution of which did not occur to him till some time afterwards. "This problem is the tendency in organic beings descended from the same stock to diverge in character as they become modified. . . . . The solution, as I believe, is that the modified offspring of all dominant and increasing forms tend to become adapted to many and highly diversified places in the economy of nature." (I, p. 84.)

It is curious that so much importance should be attached to this supplementary idea. It seems obvious that the theory of the origin of species by natural selection necessarily involves the divergence of the forms selected. An individual which varies, *ipso facto* diverges from the type of its species; and its progeny, in which the variation becomes intensified by selection, must diverge still more, not only from the parent stock, but from any other race of that stock starting from a variation of a different character. The selective process could not take place unless the selected variety was either better adapted to the conditions than the original stock, or adapted to other conditions than the original stock. In the first case, the original stock would be sooner or later extirpated; in the second, the type, as represented by the original stock and the variety, would occupy more diversified stations than it did before.

The theory, essentially such as it was published fourteen years later, was written out in 1844, and Darwin was so fully convinced of the importance of his work, as it then stood, that he made special arrangements for its publication in case of his death. But it is a singular example of reticent fortitude, that, although for the next

fourteen years the subject never left his mind, and during the latter half of that period he was constantly engaged in amassing facts bearing upon it from wide reading, a colossal correspondence, and a long series of experiments, only two or three friends were cognisant of his views. To the outside world he seemed to have his hands quite sufficiently full of other matters. In 1844, he published his observations on the volcanic islands visited during the voyage of the "Beagle." In 1845, a largely remodelled edition of his 'Journal' made its appearance, and immediately won, as it has ever since held, the favour of both the scientific and the unscientific public. In 1846, the 'Geological Observations in South America' came out, and this book was no sooner finished than Darwin set to work upon the Cirripedes. He was led to undertake this long and heavy task, partly by his desire to make out the relations of a very anomalous form which he had discovered on the coast of Chili; and, partly, by a sense of "presumption in accumulating facts and speculating on the subject of variation without, having worked out my due share of species." (II, p. 31.) The eight or nine years of labour, which resulted in a monograph of first-rate importance in systematic zoology (to say nothing of such novel points as the discovery of complemental males), left Darwin no room to reproach himself on this score, and few will share his "doubt whether the work was worth the consumption of so much time." (I, p. 82.)

In science no man can safely speculate about the nature and relation of things with which he is unacquainted at first hand, and the acquirement of an intimate and practical knowledge of the process of species-making and of all the uncertainties which underlie the boundaries between species and varieties, drawn by even the most careful and conscientious systematists* were of no less importance to the author of the 'Origin of Species' than was the bearing of the Cirripede work upon "the principles of a natural classification." (I, p. 81.) No one, as Darwin justly observes, has a "right to examine the question of species who has not minutely described many." (II, p. 39.)

In September, 1854, the Cirripede work was finished, "ten thousand barnacles" had been sent "out of the house, all over the world," and Darwin had the satisfaction of being free to turn again to his "old notes on species." In 1855, he began to breed pigeons, and to

* "After describing a set of forms as distinct species, tearing up my MS., and making them one species, tearing that up and making them separate, and then making them one again (which has happened to me), I have gnashed my teeth, cursed species, and asked what sin I had committed to be so punished." (II, p. 40.) Is there any naturalist provided with a logical sense and a large suite of specimens, who has not undergone pangs of the sort described in this vigorous paragraph, which might, with advantage, be printed on the title-page of every systematic monograph as a warning to the uninitiated?

make observations on the effects of use and disuse, experiments on seeds, and so on, while resuming his industrious collection of facts, with a view "to see how far they favour or are opposed to the notion that wild species are mutable or immutable. I mean with my utmost power to give all arguments and facts on both sides. I have a *number* of people helping me every way, and giving me most valuable assistance; but I often doubt whether the subject will not quite overpower me." (II, p. 49.)

Early in 1856, on Lyell's advice, Darwin began to write out his views on the origin of species on a scale three or four times as extensive as that of the work published in 1859. In July of the same year he gave a brief sketch of his theory in a letter to Asa Gray; and, in the year 1857, his letters to his correspondents show him to be busily engaged on what he calls his "big book." (II, pp. 85, 94.) In May, 1857, Darwin writes to Wallace: "I am now preparing my work [on the question how and in what way do species and varieties differ from each other] for publication, but I find the subject so very large, that, though I have written many chapters, I do not suppose I shall go to press for two years." (II, p. 95.) In December, 1857, he writes, in the course of a long letter to the same correspondent, "I am extremely glad to hear that you are attending to distribution in accordance with theoretical ideas. I am a firm believer that without speculation there is no good and original observation." (II, p. 108.)* In June, 1858, he received from Mr. Wallace, then in the Malay Archipelago, an 'Essay on the tendency of varieties to depart indefinitely from the original type,' of which Darwin says, "If Wallace had my MS. sketch written out in 1842 he could not have made a better short abstract! Even his terms stand now as heads of my chapters. Please return me the MS., which he does not say he wishes me to publish, but I shall, of course, at once write and offer to send it to any journal. So all my originality, whatever it may amount to, will be smashed, though my book, if ever it will have any value, will not be deteriorated; as all the labour consists in the application of the theory." (II, p. 116.)

Thus, Darwin's first impulse was to publish Wallace's essay without note or comment of his own. But, on consultation with Lyell and Hooker, the latter of whom had read the sketch of 1844, they suggested, as an undoubtedly more equitable course, that extracts from the MS. of 1844 and from the letter to Dr. Asa Gray should be communicated to the Linnean Society along with Wallace's essay. The joint communication was read on July 1, 1858, and published under the title 'On the Tendency of Species to form Varieties; and on the Perpetuation

* The last remark contains a pregnant truth, but it must be confessed it hardly squares with the declaration in the 'Autobiography' (I, p. 83) that he worked on true Baconian principles."

of Varieties and Species by Natural Means of Selection.' This was followed, on Darwin's part, by the composition of a summary account of the conclusions to which his twenty years' work on the species question had led him. It occupied him for thirteen months, and appeared in November, 1859, under the title 'On the Origin of Species by means of Natural Selection or the Preservation of Favoured Races in the Struggle of Life.'

It is doubtful if any single book, except the 'Principia,' ever worked so great and so rapid a revolution in science, or made so deep an impression on the general mind. It aroused a tempest of opposition and met with equally vehement support, and it must be added that no book has been more widely and persistently misunderstood by both friends and foes. In 1861, Darwin remarks to a correspondent, "you understand my book perfectly, and that I find a very rare event with my critics." (I, p. 313.) The immense popularity which the 'Origin' at once acquired was no doubt largely due to its many points of contact with philosophical and theological questions in which every intelligent man feels a profound interest; but a good deal must be assigned to a somewhat delusive simplicity of style, which tends to disguise the complexity and difficulty of the subject, and much to the wealth of information on all sorts of curious problems of natural history, which is made accessible to the most unlearned reader. But long occupation with the work has led the present writer to believe that the 'Origin of Species' is one of the hardest of books to master;* and he is justified in this conviction by observing that although the 'Origin' has been close on thirty years before the world, the strangest misconceptions of the essential nature of the theory therein advocated are still put forth by serious writers.

Although, then, the present occasion is not suitable for any detailed criticism of the theory, or of the objections which have been brought against it, it may not be out of place to endeavour to separate the substance of the theory from its accidents; and to show that a variety not only of hostile comments, but of friendly would-be improvements lose their *raison d'être* to the careful student. Observation proves the existence among all living beings of phenomena of three kinds, denoted by the terms heredity, variation, and multiplication. Progeny tend to resemble their parents; nevertheless all their organs and functions are susceptible of departing more or less from the average parental character; and their number is in excess of that of their parents. Severe competition for the means of living, or the struggle for existence, is a necessary consequence of unlimited multiplication; while selection, or the preservation of favourable

* He is comforted to find that probably the best qualified judge among all the readers of the 'Origin' in 1859 was of the same opinion. Sir J. Hooker writes "it is the very hardest book to read, to full profit, that I ever tried." (II, p. 242.)

variations and the extinction of others, is a necessary consequence of severe competition. "Favourable variations" are those which are better adapted to surrounding conditions. It follows, therefore, that every variety which is selected into a species is so favoured and preserved in consequence of being, in some one or more respects, better adapted to its surroundings than its rivals. In other words, every species which exists, exists in virtue of adaptation, and whatever accounts for that adaptation accounts for the existence of the species.

To say that Darwin has put forward a theory of the adaptation of species, but not of their origin, is therefore to misunderstand the first principles of the theory. For, as has been pointed out, it is a necessary consequence of the theory of selection that every species must have some one or more structural or functional peculiarities, in virtue of the advantage conferred by which, it has fought through the crowd of its competitors and achieved a certain duration. In this sense, it is true that every species has been "originated" by selection.

There is another sense, however, in which it is equally true that selection originates nothing. "Unless profitable variations . . . . . occur natural selection can do nothing" ('Origin,' Ed. I, p. 82). "Nothing can be effected unless favourable variations occur" (*ibid.*, p. 108). "What applies to one animal will apply throughout time to all animals—that is, if they vary—for otherwise natural selection can do nothing. So it will be with plants" (*ibid.*, p. 113). Strictly speaking, therefore, the origin of species in general lies in variation; while the origin of any particular species lies, firstly, in the occurrence, and secondly, in the selection and preservation of a particular variation. Clearness on this head will relieve one from the necessity of attending to the fallacious assertion that natural selection is a *deus ex machinâ*, or occult agency.

Those, again, who confuse the operation of the natural causes which bring about variation and selection with what they are pleased to call "chance" can hardly have read the opening paragraph of the fifth chapter of the 'Origin' (Ed. I, p. 131): "I have sometimes spoken as if the variations . . . . had been due to chance. This is of course a wholly incorrect expression, but it seems to acknowledge plainly our ignorance of the cause of each particular variation."

Another point of great importance to the right comprehension of the theory, is, that while every species must needs have some adaptive advantageous characters to which it owes its preservation by selection, it may possess any number of others which are neither advantageous nor disadvantageous, but indifferent, or even slightly disadvantageous. (*Ibid.*, p. 81.) For variations take place, not merely in one organ or function at a time, but in many; and thus

an advantageous variation, which gives rise to the selection of a new race or species, may be accompanied by others which are indifferent, but which are just as strongly hereditary as the advantageous variations. The advantageous structure is but one product of a modified general constitution which may manifest itself by several other products; and the selective process carries the general constitution along with the advantageous special peculiarity. A given species of plant may owe its existence to the selective adaptation of its flowers to insect fertilisers; but the character of its leaves may be the result of variations of an indifferent character. It is the origin of variations of this kind to which Darwin refers in his frequent reference to what he calls "laws of correlation of growth" or "correlated variation."

These considerations lead us further to see the inappropriateness of the objections raised to Darwin's theory on the ground that natural selection does not account for the first commencements of useful organs. But it does not pretend to do so. The source of such commencements is necessarily to be sought in indifferent variations, which remain unaffected by selection until they have taken such a form as to become utilisable in the struggle for existence.

It is not essential to Darwin's theory that anything more should be assumed than the facts of heredity, variation, and unlimited multiplication; and the validity of the deductive reasoning as to the effect of the last (that is, of the struggle for existence which it involves) upon the varieties resulting from the operation of the former. Nor is it essential that one should take up any particular position in regard to the mode of variation, whether, for example, it takes place *per saltum* or gradually; whether it is definite in character or indefinite. Still less are those who accept the theory bound to any particular views as to the causes of heredity or of variation.

That Darwin held strong opinions on some or all of these points may be quite true; but, so far as the theory is concerned, they must be regarded as *obiter dicta*. With respect to the causes of variation, Darwin's opinions are, from first to last, put forward altogether tentatively. In the first edition of the 'Origin,' he attributes the strongest influence to changes in the conditions of life of parental organisms, which he appears to think act on the germ through the intermediation of the sexual organs. He points out, over and over again, that habit, use, disuse, and the direct influence of conditions have some effect, but he does not think it great, and he draws attention to the difficulty of distinguishing between effects of these agencies and those of selection. There is, however, one class of variations which he withdraws from the direct influence of selection, namely, the variations in the fertility of the sexual union of more or less closely allied forms. He regards less fertility, or more or less

complete sterility, as "incidental to other acquired differences." (*Ibid.*, p. 245.)

Considering the difficulties which surround the question of the causes of variation, it is not to be wondered at, that Darwin should have inclined, sometimes, rather more to one and, sometimes, rather more to another of the possible alternatives. There is little difference between the last edition of the 'Origin' (1872) and the first on this head. In 1876, however, he writes to Moritz Wagner, "In my opinion, the greatest error which I have committed has been not allowing sufficient weight to the direct action of the environment, *i.e.*, food, climate, &c., independently of natural selection . . . . When I wrote the 'Origin,' and for some years afterwards, I could find little good evidence of the direct action of the environment; now there is a large body of evidence, and your case of the Saturnia is one of the most remarkable of which I have heard." (III, p. 159.) But there is really nothing to prevent the most tenacious adherent to the theory of natural selection from taking any view he pleases as to the importance of the direct influence of conditions and the hereditary transmissibility of the modifications which they produce. In fact, there is a good deal to be said for the view that the so-called direct influence of conditions is itself a case of selection. Whether the hypothesis of Pangenesis be accepted or rejected, it can hardly be doubted that the struggle for existence goes on not merely between distinct organisms, but between the physiological units of which each organism is composed, and that changes in external conditions favour some and hinder others.

After a short stay in Cambridge, Darwin resided in London for the first five years which followed his return to England; and for three years, he held the post of Secretary to the Geological Society, though he shared to the full his friend Lyell's objection to entanglement in such engagements. In fact, he used to say in later life, more than half in earnest, that he gave up hoping for work from men who accepted official duties and, especially, Government appointments. Happily for him he was exempted from the necessity of making any sacrifice of this kind, but an even heavier burden was laid upon him. During the earlier half of his voyage Darwin retained the vigorous health of his boyhood, and indeed proved himself to be exceptionally capable of enduring fatigue and privation. An anomalous but severe disorder, which laid him up for several weeks at Valparaiso in 1834, however, seems to have left its mark on his constitution; and, in the later years of his London life, attacks of illness, usually accompanied by severe vomiting and great prostration of strength, became frequent. As he grew older, a considerable part of every day, even at his best times, was spent in misery; while, not

unfrequently, months of suffering rendered work of any kind impossible. Even Darwin's remarkable tenacity of purpose and methodical utilisation of every particle of available energy could not have enabled him to achieve a fraction of the vast amount of labour he got through, in the course of the following forty years, had not the wisest and the most loving care unceasingly surrounded him from the time of his marriage in 1839. As early as 1842, the failure of health was so marked that removal from London became imperatively necessary; and Darwin purchased a house and grounds at Down, a solitary hamlet in Kent, which was his home for the rest of his life. Under the strictly regulated conditions of a valetudinarian existence, the intellectual activity of the invalid might have put to shame most healthy men; and, so long as he could hold his head up, there was no limit to the genial kindness of thought and action for all about him. Those friends who were privileged to share the intimate life of the household at Down have an abiding memory of the cheerful restfulness which pervaded and characterised it.

After mentioning his settlement at Down, Darwin writes in his Autobiography:—

"My chief enjoyment and sole employment throughout life has been scientific work; and the excitement from such work makes me, for the time, forget, or drives quite away, my daily discomfort. I have, therefore, nothing to record during the last of my life except the publication of my several books." (I, p. 79.)

Of such works published subsequently to 1859, several are monographic discussions of topics briefly dealt with in the 'Origin,' which, it must always be recollected, was considered by the author to be merely an abstract of an *opus majus*.

The earliest of the books which may be placed in this category, 'On the Various Contrivances by which Orchids are Fertilised by Insects,' was published in 1862, and whether we regard its theoretical significance, the excellence of the observations and the ingenuity of the reasonings which it records, or the prodigious mass of subsequent investigation of which it has been the parent, it has no superior in point of importance. The conviction that no theory of the origin of species could be satisfactory which failed to offer an explanation of the way in which mechanisms involving adaptations of structure and function to the performance of certain operations are brought about, was, from the first, dominant in Darwin's mind. As has been seen, he rejected Lamarck's views because of their obvious incapacity to furnish such an explanation in the case of the great majority of animal mechanisms, and in that of all those presented by the vegetable world.

So far back as 1793, the wonderful work of Sprengel had established, beyond any reasonable doubt, the fact that, in a large number

of cases, a flower is a piece of mechanism the object of which is to convert insect visitors into agents of fertilisation. Sprengel's observations had been most undeservedly neglected and well-nigh forgotten; but Robert Brown having directed Darwin's attention to them in 1841, he was attracted towards the subject, and verified many of Sprengel's statements. (III, p. 258.) It may be doubted whether there was a living botanical specialist, except perhaps Brown, who had done as much. If, however, adaptations of this kind were to be explained by natural selection, it was necessary to show that the plants which were provided with mechanisms for ensuring the aid of insects as fertilisers, were by so much the better fitted to compete with their rivals. This Sprengel had not done. Darwin had been attending to cross fertilisation in plants, so far back as 1839, from having arrived in the course of his speculations on the origin of species "that crossing played an important part in keeping specific forms constant" (I, p. 90). The further development of his views on the importance of cross fertilisation appears to have taken place between this time and 1857, when he published his first papers on the fertilisation of flowers in the 'Gardener's Chronicle.' If the conclusion at which he ultimately arrived, that cross fertilisation is favourable to the fertility of the parent and to the vigour of the offspring, is correct, then it follows that all those mechanisms which hinder self-fertilisation and favour crossing must be advantageous in the struggle for existence; and, the more perfect the action of the mechanism, the greater the advantage. Thus the way lay open for the operation of natural selection in gradually perfecting the flower as a fertilisation-trap. Analogous reasoning applies to the fertilising insect. The better its structure is adapted to that of the trap, the more will it be able to profit by the bait, whether of honey or of pollen, to the exclusion of its competitors. Thus, by a sort of action and reaction, a two-fold series of adaptive modifications will be brought about.

In 1865, the important bearing of this subject on his theory led Darwin to commence a great series of laborious and difficult experiments on the fertilisation of plants, which occupied him for eleven years, and furnished him with the unexpectedly strong evidence in favour of the influence of crossing which he published in 1876, under the title of 'The Effects of Cross and Self Fertilisation in the Vegetable Kingdom.' Incidentally, as it were, to this heavy piece of work, he made the remarkable series of observations on the different arrangements by which crossing is favoured and, in many cases, necessitated, which appeared in the work on 'The Different Forms of Flowers in Plants of the same Species' in 1877.

In the course of the twenty years during which Darwin was thus occupied in opening up new regions of investigation to the botanist and showing the profound physiological significance of the apparently

meaningless diversities of floral structure, his attention was keenly alive to any other interesting phenomena of plant life which came in his way. In his correspondence, he not unfrequently laughs at himself for his ignorance of systematic botany; and his acquaintance with vegetable anatomy and physiology was of the slenderest. Nevertheless, if any of the less common features of plant life came under his notice, that imperious necessity of seeking for causes which nature had laid upon him, impelled, and indeed compelled, him to inquire the how and the why of the fact, and its bearing on his general views. And as, happily, the atavic tendency to frame hypotheses was accompanied by an equally strong need to test them by well-devised experiments, and to acquire all possible information before publishing his results, the effect was that he touched no topic without elucidating it.

Thus the investigation of the operations of insectivorous plants, embodied in the work on that topic published in 1875, was started fifteen years before, by a passing observation made during one of Darwin's rare holidays.

"In the summer of 1860, I was idling and resting near Hartfield, where two species of Drosera abound; and I noticed that numerous insects had been entrapped by the leaves. I carried home some plants, and on giving them some insects saw the movements of the tentacles, and this made me think it possible that the insects were caught for some special purpose. Fortunately, a crucial test occurred to me, that of placing a large number of leaves in various nitrogenous and non-nitrogenous fluids of equal density; and as soon as I found that the former alone excited energetic movements, it was obvious that here was a fine new field for investigation." (I, p. 95.)

The researches thus initiated led to the proof that plants are capable of secreting a digestive fluid like that of animals, and of profiting by the result of digestion; whereby the peculiar apparatuses of the insectivorous plants were brought within the scope of natural selection. Moreover, these inquiries widely enlarged our knowledge of the manner in which stimuli are transmitted in plants, and opened up a prospect of drawing closer the analogies between the motor process of plants and those of animals.

So with respect to the books on 'Climbing Plants' (1875), and on the 'Power of Movement in Plants' (1880), Darwin says;—

"I was led to take up this subject by reading a short paper by Asa Gray, published in 1858. He sent me some seeds, and on raising some plants I was so much fascinated and perplexed by the revolving movements of the tendrils and stems, which movements are really very simple, though appearing at first sight very complex, that I procured various other kinds of climbing plants and studied the whole subject . . . . Some of the adaptations displayed by climbing plants

are as beautiful as those of orchids for ensuring cross-fertilisation." (I, p. 93.)

In the midst of all this amount of work, remarkable alike for its variety and its importance, among plants, the animal kingdom was by no means neglected. A large moiety of 'The Variation of Animals and Plants under Domestication' (1868), which contains the *pièces justificatives* of the first chapter of the 'Origin,' is devoted to domestic animals, and the hypothesis of 'pangenesis' propounded in the second volume applies to the whole living world. In the 'Origin' Darwin throws out some suggestions as to the causes of variation, but he takes heredity, as it is manifested by individual organisms, for granted, as an ultimate fact; pangenesis is an attempt to account for the phenomena of heredity in the organism, on the assumption that the physiological units of which the organism is composed give off gemmules, which, in virtue of heredity, tend to reproduce the unit from which they are derived.

That Darwin had the application of his theory to the origin of the human species clearly in his mind in 1859, is obvious from a passage in the first edition of 'The Origin of Species.' (Ed. 1, p. 488.) "In the distant future I see open fields for far more important researches. Psychology will be based on a new foundation, that of the necessary acquirement of each mental power and capacity by graduation. Light will be thrown on the origin of man and his history." It is one of the curiosities of scientific literature, that, in the face of this plain declaration, its author should have been charged with concealing his opinions on the subject of the origin of man. But he reserved the full statement of his views until 1871, when the 'Descent of Man' was published. The 'Expression of the Emotions' (originally intended to form only a chapter in the 'Descent of Man') grew into a separate volume, which appeared in 1872. Although always taking a keen interest in geology, Darwin naturally found no time disposable for geological work, even had his health permitted it, after he became seriously engaged with the great problem of species. But the last of his labours is, in some sense, a return to his earliest, inasmuch as it is an expansion of a short paper read before the Geological Society more than forty years before, and, as he says, "revived old geological thoughts" (I, p. 98). In fact, 'The Formation of Vegetable Mould through the Action of Worms,' affords as striking an example of the great results produced by the long continued operation of small causes as even the author of the 'Principles of Geology' could have desired.

In the early months of 1882 Darwin's health underwent a change for the worse; attacks of giddiness and fainting supervened, and on the 19th of April he died. On the 24th, his remains were interred in Westminster Abbey, in accordance with the general feeling that such

a man as he should not go to the grave without some public recognition of the greatness of his work.

Mr. Darwin became a Fellow of the Royal Society in 1839; one of the Royal Medals was awarded to him in 1853, and he received the Copley Medal in 1864. The 'Life and Letters,' edited with admirable skill and judgment by Mr. Francis Darwin, gives a full and singularly vivid presentment of his father's personal character, of his mode of work, and of the events of his life. In the present brief obituary notice, the writer has attempted nothing more than to select and put together those facts which enable us to trace the intellectual evolution of one of the greatest of the many great men of science whose names adorn the long roll of the Fellows of the Royal Society.

T. H. H.

Mr. Thomas Blizard Curling, F.R.C.S., F.R.S., died at Cannes, on the 4th of March, 1888, in the 78th year of age, of a severe attack of pneumonia or congestion of the lungs, caused by chill.

This distinguished surgeon was born in 1811, and resided in London during the greater part of his professional life, which was one of continued scientific and public utility. The value of his contributions to surgery and pathology, his great eminence as a surgeon and clinical teacher, and his upright, just, and honourable character, not only placed him in the foremost rank of his profession, but secured for him the affection and esteem of the numerous friends who deplore his loss.

Mr. Curling had retired from the active duties of his profession as Senior Surgeon of the London Hospital in 1869, but continued to practise until within the last ten years, which were spent in well-earned rest at Brighton, varied by occasional visits to the Riviera, where, as has been stated, his career was brought to a sudden close by a severe pulmonary attack. He obtained professional distinction early in life; at the age of twenty-two he was appointed Assistant-Surgeon of the London Hospital; in this he appears to have been partly aided by the influence of his uncle, Sir W. Blizard, who thus happily had the means of placing the opportunity of advancement, which was so readily seized and so fully utilised, within the grasp of the young surgeon whose brilliant subsequent career proved how justly its early promise had been estimated by those who appointed him to so important a post.

Mr. Curling's career as a hospital surgeon and teacher of surgery was one of continued progress and success. A recent notice of him says:—"Perhaps nowhere was his character more apparent than while ward-visiting at the London Hospital. His methodical and punctual

habits passed into a proverb with the students, for he usually entered the gates as his visiting hour struck. A strict disciplinarian, he was a terror to the slovenly dresser, but an object of respect and admiration to the zealous. Freely blaming, if blame were due, he never withheld praise when such was deserved. Exact and punctilious in detail himself, he evinced his strong sense of duty to the patient by examining into the smallest minutiæ of dressing and note-taking. Possessing a sound and well-balanced judgment, backed by great clinical experience, he did not permit theories to be based on insufficient bases. His practice and his teaching were not at variance; both were sound, upright, and just."

Mr. Curling was appointed Lecturer on Surgery in 1846, and became Full Surgeon of the London Hospital in 1849, from which office he retired in 1869, retaining that of Consulting Surgeon till his death. He was appointed Examiner in Surgery to the London University in 1859, Member of the Council of the Royal College of Surgeons in 1864, Examiner in 1871, and filled the high office of President in 1873. He had been elected a Fellow of the Royal Society as early as 1850.

His large and varied experience is stamped on his written works. His earliest investigations were on tetanus, which were rewarded by the Jacksonian prize in 1834. This sound work was followed by many communications of interest and importance to the Royal Medico-Chirurgical and Pathological Societies, comprising amongst them that upon Duodenal Ulceration as a consequence of burns. Towards 1855 the subject of his articles tended rather to the illustration of diseases of the testes and rectum, and his wide experience in these sections of surgery was of much benefit to those who suffered from these affections. His works on Diseases of the Testis and on Diseases of the Rectum, each of which reached a fourth edition, are standard authorities on the subjects of which they treat.

He was a most courteous, amiable man; undemonstrative in manner, but sincere and true in his friendships and feelings. His character has justly been described as "one of singular honesty and straightforwardness; he had a kind heart, and secured and kept the deep respect of all who knew him."

Mr. Curling had two sons, both of whom, as well as their mother, predeceased him.

J. F.

By the death of PHILIP HENRY GOSSE the Society has lost not only a many-sided and experienced naturalist, but one who did more than almost any of his scientific contemporaries to popularise the study of natural objects.

Mr. Gosse was born at Worcester in 1810—his father, a miniature painter of some note in his day. He was educated, in part at least, at the Blandford Grammar School, and at seventeen was sent out to Newfoundland as a clerk in a business house. After eight years of commercial life he settled in Canada as a farmer, but the venture did not prove successful, and he returned to England. In 1838 he went south through the United States, and was engaged as a schoolmaster in Alabama; subsequently he resided for some time in Jamaica as a professional naturalist, and then, having definitely adopted natural history and literature as a profession, he returned to settle in England. The roving life of his earlier years afforded wide opportunities for natural history pursuits, and his early works show evidence at least of acute powers of observation. 'The Canadian Naturalist' (1840) and 'The Birds of Jamaica' (1851) were perhaps his most important contributions during this period, but he had published also a number of zoological manuals and other books of more popular character.

From this time, however, Mr. Gosse devoted himself more particularly to the British marine fauna and flora. He was an assiduous collector and, simultaneously perhaps with the late Mr. Warrington, devised the marine aquarium, as a means of observing the habits and economy of marine shallow-water organisms. The idea was taken up by the Zoological Society, who, in 1853, constructed tanks on a considerable scale in their gardens in Regent's Park. 'A Naturalist's Rambles on the Devonshire Coast,' a little handbook to 'The Aquarium' (1853–4), and other works of similar bearing published about the same time, attracted much attention and, as a practical result, aquaria became common, and the collection of objects for them a popular sea-side amusement. Of greater importance from a scientific point of view was his 'Manual of Marine Zoology' (1855–6); two small volumes, copiously illustrated with outline drawings—a work extremely useful in its day.

Mr. Gosse's subsequent contributions to scientific literature were less frequent but of more original character. His name will probably be best remembered as the author of the 'Actinologia Britannica,' a history of the sea-anemones and corals of the British Islands, which still, after the lapse of nearly thirty years, maintains its authoritative position. Of later times his attention was more particularly directed to the Rotifera, and the results of his observations up to 1886 were embodied in an important monograph of the group, published conjointly with Dr. C. T. Hudson. The Society's 'Catalogue of

Scientific Papers' contains a list of nearly sixty memoirs from his pen between 1843 and 1867, and this number would require considerable addition to include those of recent years. But the bulk of his literary labour was expended on works of more popular nature. These were very numerous, and embraced a great variety of subjects; the style was generally very happily chosen, and they were marked by the same accuracy as his more strictly scientific writings. Much of the interest and value of Mr. Gosse's contributions to science is due to their admirable illustration, the author's facility and precision with pencil and brush, which lasted late into old age, being no doubt in part an inherited gift.

Mr. Gosse was elected a Fellow of the Society in 1856. His decease took place at Marychurch on the 23rd of April, 1888, in his 79th year. For many years he had led a secluded life, of which his friends were kept aware by his occasional contributions to the scientific journals.

H. B. B.